of the State is due not to the general frequency of thunderstorms in that region, but to the occurrence of some one specially severe storm.

6. Mr. Allen gives no data by which one can estimate the relative safety of buildings with and without lightning rods, as every one of those injured had no lightning rod. Some years ago several of our American firms manufacturing lightning rods, in their replies to the circulars of the Lightning Rod Conference, stated that no buildings protected by their rods had ever been injured. Doubtless this was an exaggeration, and yet the general conclusion to which that conference arrived still holds good, i. e., that injuries are inflicted only when the lightning rod is imperfect. If it be properly made and placed in the proper position, and in perfect electrical connection with the earth, and be terminated at the top with fine points, then the edifice protected by it with all that it contains will be safe. All accidents may be said to be due to a neglect of these simple elementary principles; "there is no authentic case on record where a properly constructed conductor failed to do its duty."—ED.]

NOTES BY THE EDITOR.

BALL LIGHTNING.

The Editor has received the two letters following from Mr. C. N. Crotsenburg, Crow Agency, Montana, dated August 22 and September 6, 1898, respectively, and submits his own suggested explanation, but thinks it worth while to publish the whole in full, in order that the attention of others may be drawn to the subject, since, if his own explanation is correct, the phenomenon must be frequently seen by others:

As you expressed a desire to hear from those who had observed "ball lightning" at various times, I will relate an experience which befell me in the summer of 1896. I was then employed as a railway postal clerk on the line of the Chicago, Rock Island and Pacific Railway, between Davenport, Iowa, and Leavenworth, Kans.

One very dark night, about the middle of the summer, our train was going northward between Trenton, Mo., and Lineville, Iowa. Just before reaching Princeton, in Mercer County, a heavy rain began falling, which necessitated the closing of the doors on the east side of the mail car. Soon after leaving that station, at 10:35 p. m., my companion (Mr. R. C. Corbin) lay down for a short sleep. The work being very light that night, I sat in a chair, looking out of the car door to the west. The darkness was intense; not a ray of light was visible from any point, except from the train. When a few miles out from Princeton, and while traveling almost due north, I observed a peculiar light low down on the western horizon. It appeared to be perfectly round and about a foot in diameter, of a dull rose color, or, possibly, like a piece of live coal. When first observed it seemed to be floating within a hundred feet of the earth, but soon rose to a height about midway between the horizon and the zenith. For a time it floated very steadily, but soon began to oscillate up and down, at times even dropping out of sight behind hills. The wind was quite strong from the east, but the light traveled in an almost true north course. Its speed varied, sometimes seeming to outrun the train considerably, and at others it would fall behind, but never far enough to be lost to sight. Most of the time it appeared to be nearly abreast of the train and apparently from half a mile to a mile distant. Soon after it was first observed by me, my companion arose, and we both watched it closely until the town of Lineville, Iowa, was reached. There it passed out of sight behind the depot, and we saw it no more. During all the time it was in sight there was a heavy fall of rain, but very little lightning. It seemed to follow a course parallel to the Grand River, moving upstream. We had no idea at the time what caused the light, but I have since become convinced that it was "ball lightning."

* * * * * * *

Replying to your letter of August 30, and referring to my own of August 22: I have no means of ascertaining the date of the occurrence related. It made a very deep impression upon me at the time, and Mr. Corbin and myself often talked about it afterward, but I made no note of the date. We made many inquiries concerning the phenomenon which we observed, but never obtained a satisfactory explanation. We thought of distant electric light, but found that none existed within the range of our vision on that portion of the road. Since beginning this I remember that we remarked that if it had occurred a few nights before we should have felt certain that it was the light from a balloon sent up from some Fourth of July celebration, so probably it was within a week of July 4, 1896.

But even then, it would have been remarkable, as the light traveled almost directly north and kept an even course, while the wind blew quite strongly from the east. It was so very strange that I should never have mentioned it, even to my friends, had it not been corroborated by a reliable witness. I have sought for an explanation whenever and wherever I have had an opportunity, and from what I have been able to learn I had come to the conclusion that it was most probably "ball lightning."

Although my conclusions may be entirely erroneous, I have no more plausible theory to advance.

The fact that it was observed by both of us at the same time, and had the same appearance to his eyes as it had to mine before he saw it is very good evidence that it was a reality and not an hallucination. He observed it as he was passing the open door, and before I had spoken to him. In fact the thing was so unreal that I hesitated to speak of it, fearing that it was some freak of my imagination, but when he too saw it the same, I could no longer doubt its existence as a reality, and we both observed it closely while the train was running at least 15 miles. When it disappeared it was at least a mile from us, as the buildings of the town were plainly visible and it was some distance farther west than any of them.

There has always been a great deal of doubt as to whether the phenomenon known as "ball lightning" might not in some way be an optical delusion. The phenomenon rarely occurs, and it is therefore rarely seen by experienced electricians, and the latter were therefore justly skeptical. But within the past twenty years, so many instances have been recorded, some of them by observers of undoubted ability, that there can be no doubt but what this form of electrical discharge can exist, although at present we have no idea of its relation to the other forms of electricity. The following instances are recorded in the famous report of the lightning rod conference, compiled by ten or twelve members of prominent scientific organizations in England, edited by the well-known meteorologist G. J. Symons, and published in 1882:

Page 99. Near Strasburg, Germany, a discharge of globular lightning traversed a horizontal distance of 919 yards, passing below the top of a building which had three good conductors upon it, and struck a distant chestnut tree.

Page 205. D. Morgan, Master of the bark Southern Queen, on December 30, 1879, in the midst of a terrific squall, saw a ball of fire descend from the mizen and go over the port side of the vessel.

Page 242. Prof. P. G. Tait, of Edinborough, says that fire ball or globe lightning undoubtedly exists, and is probably due to a species of natural leyden jar very highly charged, which no lightning rod can destroy, except perhaps a close net work of stout copper wires.

The preceding account of a phenomenon by Messrs. Crotsenburg and Corbin does not harmonize with the ordinary descriptions of ball lightning well enough to justify applying that term to it, and the editor believes that some other explanation of the phenomenon must be found. If there were a mass of falling rain, or fog, or haze at a little distance west of the train in which they were traveling, or in fact if there were groves or forests, the leaves of which were covered with rain drops, these would undoubtedly send back to the observer's eye a faint reflection or more properly an antisolar corona, which would be barely visible on a dark night. It would necessarily appear to float along with the train, as the Crotsenburg phenomenon did. There can be no doubt but what the light observed in this case was some form of reflection of the light of the train itself, as it certainly had none of the characteristics of ball lightning.

THE MEASUREMENT OF THE WIND.

The velocity of the wind is usually measured by means of some form of windmill apparatus, such as the Robinson ane-

mometer or the rotating blades of Combes' wind meter or the curved plates of the Richard anemometer, or of Dine's helicoid anemometer. On the other hand the wind pressure is observed by exposing a plane plate, normal to the direction of the wind, or sometimes a sphere is substituted for the plate; in Wild's pendulum anemometer the plane plate swings like a pendulum out of the vertical position for calms, up to as high an angle as the force of the wind requires. This latter form of apparatus gives us some idea of the force of the gust of wind, the gust may be but momentary, being only the front of a "pulse of pressure," or of a revolving eddy; its force is due to the very rapid motion, through a very short distance, of a small mass of air. When such a gust strikes the cups of the Robinson anemometer they are whirled about with great speed, and retain that speed for some time after the gust has ceased. The sum total of the movement shown by this anemometer is distributed over a longer time than the gust itself endures, and the extreme velocity attained is less than that of the gust at its maximum intensity, but the average velocity shown by the cups is greater than belongs to the gust as a whole. If, therefore, we compare the records of the maximum pressure and the maximum velocity recorded in the open air we find that the velocity seems to be less than is appropriate to the pressures.

If, on the other hand, the velocity and the pressure anemometers are set up side by side in a current of perfectly uniform wind, or failing that, if they are established on a uniformly revolving whirling arm in a room with still air, then, for this case of uniform motion, the pressures and the velocities harmonize perfectly. This latter is the ordinary method of comparing anemometers in order to deduce figures necessary to reduce the indications of one instrument to that of another. Nearly all kinds of anemometers will agree very closely when they are compared in a perfectly uniform stream of air, but they will show wide differences in gusty winds. These differences will increase in proportion to the friction within the apparatus, and especially in proportion to the inertia, or more properly the moment of inertia.

This gusty nature of the atmosphere has of course been known from ancient times. In fact the motion of the air in what we call a steady wind is not linear but a mass of whirls and eddies, as may be seen by any one who will watch the gyrations of a snowflake. Professor Marvin seems to have been the first to perceive that the presence of these gusts explains the discrepancy between the action of the anemometers in the open air as compared with their behavior in the steady wind of the whirling machine. He has explained this matter and given accurate determinations of its amount in numerous publications between February, 1889, and December, 1890. In order to show the effect of gustiness most clearly Professor Marvin employed some very light anemometers of paper of the same size as the regulation Robinson anemometer, having previously used others of much smaller size. The sluggishness of these anemometers depends upon the moment of inertia, viz, the mass multiplied by the radius of gyration, and his smallest anemometers had not one one-thousandth part of the sluggishness of the regulation apparatus. The latter being set into rapid motion by the gusts showed slightly larger average velocities than the smaller instruments which showed higher maximum velocities, but stopped their rapid motion as soon as the gusts went by.

Professor Marvin concludes that after determining the error of an anemometer in a steady wind by means of a whirling apparatus, one must then still further determine its errors in gusty winds; but this latter is well nigh impossible, or rather useless, because the gusts are of such infinite variety, as to severity and lengths and vacillation. It would seem possible to establish side by side in the open air several anemometers of very diverse moments of inertia, in order, by the comparison of their records, to determine the general influence of the gustiness of any particular location or wind. In general, Professor Marvin finds for the average character of the gusts at Washington a set of corrections that are embodied in his pamphlet on anemometry. His last report on this subject, dated October 4, 1890, is published on pages 691–698 of the Annual Report of the Chief Signal Officer of 1890, and as his results are frequently called for, we quote from that report the following:

The formulæ derived for the Signal Service anemometer having cups 4 inches in diameter on arms 6.72 inches long are—

(*a*) $V = 0.225 + 3.143\,v\,0.0362\,v^2$—(for whirling machine.)

(*b*) $V = 0.263 + 2.953\,v\,0.0407\,v^2$—(*a*, reduced to open air.)

V is velocity of winds in miles per hour; v is velocity of centers of cups in miles per hour.

Equation (*b*) may be considered as the equation of the regulation Signal Service anemometer when exposed to the variable wind of the open in Washington, while equation (*a*) is for the same anemometer exposed to perfectly steady winds.

[and a moment of inertia of about 117,000 grammes centimeters.]

In order to overcome certain defects in this quadratic equation which would prevent its being applied to very high velocities, Professor Marvin deduced and recommends the following logarithmic form:

(*c*) $\log V = 0.509 + 0.9012 \log v$

With this formula he computed a table of wind velocities as indicated by Robinson anemometers, converted into true velocities especially applicable to the gusty winds of Washington, from which we make the following abstract:

Weather Bureau anemometers. Indicated velocity.	True velocity.
Miles per hour.	*Miles per hour.*
10	9.6
20	17.8
30	25.7
40	33.3
50	40.8
60	48.0
70	55.2
80	62.3
90	69.2

The importance of inertia in gusty air is also dwelt on by Professor Langley in his memoir On the Internal Work of the Wind, published in 1893, in the prosecution of which he was assisted by Mr. G. E. Curtis, formerly of the Signal Office, and made use of the paper anemometers constructed by Professor Marvin.

The last volume of the Archives of the German Marine Observatory at Hamburg, contains an elaborate memoir by Dr. George Neumayer, on anemometer studies, in which he reviews the whole subject, and concedes the correctness of Professor Marvin's results, which have also been further confirmed by the investigation of Dr. C. Chree, Director of the Kew Observatory.

NOTES FROM THE AUGUST REPORTS OF THE CLIMATE AND CROP SECTIONS.

ALABAMA.

The regular meteoric shower of August will frequently bring large meteors, and this seems to be especially the case in Alabama, where especially brilliant meteors were observed on the 9th at 8:40 p. m., at Selma, and on the 16th at 9.30 p. m., at Montgomery. The latter was so brilliant as to outshine the public electric lights.

ARIZONA.

The voluntary observer at Fort Mojave reports that on the 28th there was a heavy rain and furious wind, but the latter